VIII. *An Account of a new Eudiometer.*
By Mr. Cavendiſh, *F. R. S.*

Read January 16, 1783.

DR. PRIESTLEY's diſcovery of the method of determining the degree of phlogiſtication of air by means of nitrous air, has occaſioned many inſtruments to be contrived for the more certain and commodious performance of this experiment; but that invented by the Abbé FONTANA is by much the moſt accurate of any hitherto publiſhed. There are many ingenious contrivances in his apparatus for obviating the ſmaller errors which this experiment is liable to; but the great improvement conſiſts in this, that as the tube is long and narrow, and the orifice of the funnel not much leſs than the bore of the tube, and the meaſure is made ſo as to deliver its contents very quick, the air riſes ſlowly up the tube in one continued column; ſo that there is time to take the tube off the funnel, and to ſhake it before the airs come quite in contact, by which means the diminution is much greater and much more certain than it would otherwiſe be. For inſtance, if equal meaſures of nitrous and common air are mixed in this manner, the bulk of the mixture will, in general, be about one meaſure; whereas, if the airs are ſuffered to remain in contact about one-fourth of a minute before they are ſhaken, the bulk of the mixture will be hardly leſs than one meaſure and two-tenths, and will be very different according as it is ſuffered to remain

remain a little more or a little leſs time before it is ſhaken. In like manner, if through any fault in the apparatus, the air riſes in bubbles, as in that caſe it is almoſt impoſſible to ſhake the tube ſoon enough, the diminution is leſs than it ought to be.

Another great advantage in this manner of mixing is, that thereby the mixture receives its full diminution in the ſhort time during which it is ſhaken, and is not ſenſibly altered in bulk after that; whereas, if the airs are ſuffered to remain ſome time in contact before they are ſhaken, they will continue diminiſhing for many hours.

The reaſon of the abovementioned differences ſeems to be, that in the Abbé FONTANA's method the water is ſhaken briſkly up and down in the tube while the airs are mixing, whereby each ſmall portion of the nitrous air muſt be in contact with water, either at the inſtant it mixes with the common air, or at leaſt immediately after; and it ſhould ſeem, that when the airs are in contact with water during the mixing, the diminution is much greater and more certain than when there is no water ready to abſorb the nitrous acid produced by the mixture. This induced me to try whether the diminution would not be ſtill more certain and regular if one of the two kinds of air was added ſlowly to the other in ſmall bubbles, while the veſſel containing the latter was kept continually ſhaking. I was not diſappointed in my expectations, as, I think, this method is really more accurate than the Abbé FONTANA's; and, moreover, in the courſe of my experiments I had occaſion to obſerve a circumſtance which is neceſſary to be attended to by thoſe who would examine the purity of air with exactneſs by any kind of eudiometer, beſides ſome others which tend very much to explain many of the phenomena attending the mixture of common and nitrous air.

The apparatus I uſe is as follows. A (fig. 1.) is a cylindrical glaſs veſſel, with braſs caps at top and bottom; to the upper cap is fitted a braſs cock B; the bottom cap is open, but is made to fit cloſe into the braſs ſocket D*d*, and is fixed in it in the ſame manner as a bayonet is on a muſquet. The ſocket D*d* has a ſmall hole E in its bottom, and is faſtened to the board of my tub by the bent braſs F*f*G, in ſuch manner that *b*, the top of the cock, is about half an inch under water; conſequently if the veſſel A is placed in its ſocket, with any quantity of air in it, and the cock is then opened, the air will run out by the cock, but will do ſo very ſlowly, as it can eſcape no faſter than the water can enter by the ſmall hole E to ſupply its place.

Beſides this veſſel, I have three glaſs bottles like M (fig. 2.) each with a flat braſs cap at bottom to make it ſtand ſteady, and a ring at top to ſuſpend it by, and alſo ſome meaſures of different ſizes ſuch as B (fig. 3.); theſe are of glaſs with a flat braſs cap at bottom and a wooden handle. In uſing them they are filled with the air wanted to be meaſured, and then ſet upon the braſs knob C fitted upon the board of my tub below the ſurface of the water, which drives out ſome of the air, and leaves only the proper quantity. This meaſure is eaſier made, and more expeditious in uſing, than the Abbé FONTANA'S, and, I believe, is equally accurate; but if it was not it would not ſignify, as I determine the exact quantity of air uſed by weight.

There are two different methods of proceeding which I have uſed; the firſt is to add the reſpirable air ſlowly to the nitrous; and the other, to add the nitrous air in the ſame manner to the reſpirable. The firſt is what I have commonly uſed, and which I ſhall firſt deſcribe. In this method a proper quantity of nitrous air is put into one of the bottles M, by means of one of the meaſures above deſcribed, and a proper quantity of reſpirable is let into the veſſel A, by firſt filling it with this air, and then ſetting it on the knob C, as was done by the meaſure. The veſſel A is then fixed in the ſocket, and the bottle M placed with its mouth over the cock. Then on opening the cock, the air in the veſſel A runs ſlowly in ſmall bubbles into the bottle M, which is kept ſhaking all the time by moving it backwards and forwards horizontally while the mouth ſtill remains over the cock.

Notwithſtanding the precautions uſed by the Abbé FONTANA in meaſuring the quantity of air uſed, I have ſometimes found that method liable to very conſiderable errors, owing to more water ſticking to the ſides of the meaſure and tube at one time than at another: for this reaſon I determine the quantities of air uſed and the diminution, by weighing the veſſels containing it under water in this manner. From one end of a balance, placed ſo as to hang over the tub of water, is ſuſpended a forked wire, to each end of which fork is fixed a fine copper wire; and in trying the experiment the veſſel A, with the reſpirable air in it, is firſt weighed, by ſuſpending it from one of theſe copper wires, in ſuch manner as to remain intirely under water. The bottle M, with the proper quantity of nitrous air in it, is then hung on in the ſame manner to the other wire, and the weight of both together found. The air is then let out of the veſſel A into the bottle M, and the weight of both veſſels together found again, by which the diminution of bulk which they ſuffer on mixing is known. Laſtly, the bottle M is taken off, and the veſſel A weighed again by itſelf, which gives the quantity of reſpirable air uſed. It is needleſs to determine the quantity of nitrous air by weight; becauſe, as the quantity uſed is always ſufficient to produce the full diminution, a ſmall difference in the quantity makes no ſenſible difference in the diminution *. In this manner of determining the quantities by weight, care ſhould be taken to proportion the lengths of the copper wires in ſuch manner that the ſurface of the water in A and M ſhall be on the

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* Mr. DE SAUSSURE alſo determines the quantity of air which he uſes by weight; but does it by weighing the veſſels containing it in air. This method is liable to ſome inaccuracy, as the air in the veſſel is apt to be compreſſed by putting in the ſtopper; though, I believe, that, if care is taken to puſh in the ſtopper ſlowly, the error ariſing from thence is but ſmall. It is alſo leſs expeditious than weighing them under water, as ſome time is neceſſarily loſt in wiping the wet off the veſſels; but, on the other hand, it requires leſs apparatus, which makes it fitter for a portable apparatus as Mr. DE SAUSSURE's was. If any gentleman is deſirous of adapting this method of determining the quantities to the above deſcribed manner of mixing the airs, nothing more is required than to have glaſs ſtoppers fitted to the veſſel A and to the bottle M.

It is needleſs to mention, that in both theſe methods no ſenſible error can ariſe from any difference in the ſpecific gravity of the air; for the thing found by weighing the veſſel is the difference of weight of the included air and of an equal bulk of water, which, as no air is leſs than 500 times lighter than water, is very nearly equal to the weight of a quantity of water, equal in bulk to the included air.

It muſt be obſerved, that a common balance is not convenient for weighing the veſſels of air under water, without ſome addition to it; for the lower the veſſel of air ſinks under the water, the more the air is compreſſed, which makes the veſſel heavier, and thereby cauſes that end of the beam to preponderate. This makes it neceſſary either to have the index placed below the beam, as in many aſſay balances; or by ſome other means to remove the center of gravity of the beam ſo much below the center of ſuſpenſion as to make the balance vibrate, notwithſtanding the tendency which the compreſſibility of the air in the veſſels has to prevent it.

the ſame level when both have the uſual quantity of air in them, as otherwiſe ſome errors will ariſe from the air being more compreſſed in one than in the other. This precaution indeed does not intirely take away the error, as the level of the water in M is not the ſame after the airs are mixed as it was before; but in veſſels of the ſame ſize as mine, the error ariſing from thence can never amount to the 500th part of the whole, which is not worth regarding; and indeed if it were much greater, it would be of very little conſequence, as it would be always the ſame in trying the ſame kind of air.

There are ſeveral contrivances which I uſe, in order to diminiſh the trouble of weighing the veſſels; but I omit them, as the deſcription would take up too much room.

The veſſel A holds 282 grains of water, and is the quantity which I ſhall diſtinguiſh by the name of one meaſure. I have three bottles for mixing the airs in, with a meaſure B for the nitrous air adapted to each. The firſt bottle holds three meaſures, and the correſponding meaſure 1¼; the ſecond bottle holds ſix, and the correſponding meaſure 2½; and the third bottle holds 12, and the correſponding meaſure 5. The firſt bottle and meaſure is uſed in trying common air, or air not better than that; the two other in trying dephlogiſticated air. The quantity of reſpirable air uſed, as was ſaid before, is always the ſame, namely, one meaſure; conſequently, in trying common air I uſe 1¼ meaſures of nitrous air to one of common; and in trying very pure dephlogiſticated air I uſe five meaſures of nitrous air to one of the dephlogiſticated. I believe there is no air ſo much dephlogiſticated as to require a greater proportion of nitrcus than that. The way by which I judge whether the quantity of nitrous air uſed is ſufficient, is by the bulk of the two airs when mixed; for if that is not leſs than one meaſure, that is, than the reſpirable air alone, it is a ſign that the quantity of nitrous air is ſufficient, or that it is ſufficient to produce the full diminution, unleſs it is very impure.

Though the quantity of reſpirable air uſed will be always nearly the ſame, as being put in by meaſure; yet it will commonly be not exactly ſo, for which reaſon the obſerved diminution will commonly require ſome correction: for example, ſuppoſe that the obſerved diminution was 2.353 meaſures, and that the quantity of reſpirable air was found to be .985 of a meaſure; then the obſerved diminution muſt be increaſed by $\frac{15}{1000}$ of the whole or .035, in order to have the true diminution, or that which would have been produced if the reſpirable air uſed had been exactly one meaſure; conſequently, the true diminution is 2.388

The method of weighing, deſcribed in p. 109. is that which I uſe in trying air much different in purity from common air; but in trying common air, I uſe a ſhorter method, namely, I do not weigh the veſſel A at all, but only weigh the bottle M with the nitrous air in it; then mix the airs, and again weigh the ſame bottle with the mixture in it, and find the increaſe of weight. This, added to one meaſure, is very nearly the true diminution, whether the quantity of common air uſed was a little more or a little leſs than one meaſure. The reaſon of this is, that as the diminution produced on mixing common and nitrous air is only a little greater than the bulk of the common air, the bulk of the mixture will be very nearly the ſame, whether the bulk of the common air is a little greater or a little leſs than one meaſure: for example, let us firſt ſuppoſe, that the quantity of common air uſed is exactly one meaſure, and that the diminution of bulk on mixing is 1.08 of a meaſure, then muſt the increaſe of weight of the bottle M, on adding the common air, be .08 of a meaſure. Let us now ſuppoſe, that the quantity of common air uſed is 1.02 of a meaſure, then will the diminution, on adding the common air, be $1.08 \times \frac{1.02}{1.00}$ or 1.1016 of a meaſure, and conſequently the increaſe of weight of the bottle M will be 1.1016 − 1.02 or .0816 of a meaſure, which is very nearly the ſame as if the common air uſed had been exactly one meaſure.

In the ſecond method of proceeding, or that in which the nitrous air is added to the reſpirable, I uſe always the ſame bottle, namely, that which holds three meaſures, and uſe always one meaſure of reſpirable air; and in trying common air uſe the ſame veſſel A as in the firſt method; but for dephlogiſticated air I uſe one that holds $3\frac{3}{4}$ meaſures.

In trying the experiment I firſt weigh the bottle M without any air in it, and then weigh it again with the reſpirable air in it, which gives the quantity of reſpirable air uſed. I next put the nitrous air into the veſſel A, and weigh that and the bottle M together, and then having mixed the airs, weigh them again, which gives the diminution.

From what has been juſt ſaid, it appears, that in this method of proceeding I uſe a leſs quantity of nitrous air in trying the ſame kind of reſpirable air than in the former; the reaſon of which is, that the ſame quantity of nitrous air goes further in phlogiſticating a given quantity of reſpirable air in this than in the former method, as will be ſhewn further on.

In both theſe methods I expreſs the teſt of the air by the diminution which they ſuffer in mixing; for example, if the diminution on mixing them is two meaſures and $\frac{353}{1000}$, I call its teſt 2.353, and ſo on.

In the firſt method of proceeding I found, that the diminution was ſcarce ſenſibly leſs when I uſed one meaſure of nitrous air than when I uſed a much greater quantity; ſo that one meaſure is ſufficient to produce the full diminution. I chuſe, however, to uſe $1\frac{1}{4}$, for fear the nitrous air may be impure; $\frac{7}{8}$ths of a meaſure of nitrous air produced about $\frac{15}{16}$, and $\frac{3}{4}$ths of a meaſure about $\frac{7}{9}$ths of the full diminution.

I found alſo, that there was no ſenſible difference in the diminution whether the orifice by which the air paſſed out of the veſſel A into the bottle M was only $\frac{1}{25}$th of an inch in diameter, or whether it was $\frac{1}{8}$th of an inch; that is, whether the air eſcaped in ſmaller or larger bubbles. The diminution was rather leſs when the bottle was ſhook gently than when briſkly; but the difference between ſhaking it very gently and as briſkly as I could was not more than $\frac{1}{100}$dth of a meaſure. But if it was not ſhaken at all the diminution was remarkably leſs, being at firſt only ,9; in about 3′, indeed, it increaſed to ,93, and after being ſhaken for about a minute it increaſed to ,99; whereas, when the bottle was ſhaken gently, the diminution was 1,08 at firſt mixing, and did not increaſe ſenſibly after that time. The difference proceeding from the difference of time which the air took up in paſſing into the bottle was rather greater; namely, in ſome trials, when it took up 80″ in paſſing, the diminution was $\frac{5}{100}$dths greater than when it took up only 22″, and about $\frac{2}{100}$dths greater than when it took up 45′; in ſome other trials, however, the difference was leſs. It appears, therefore, that the difference ariſing from the difference of time which the air takes up in paſſing into the bottle is conſiderable; but, as with the ſame hole in the plate D*d* it will take up always nearly the ſame time, and as it is eaſy adjuſting the ſize of the hole, ſo as to make it take up nearly the time we deſire, the error proceeding from thence is but ſmall. The time which it took up in paſſing in my experiments was uſually about 50″.

The difference proceeding from the difference of ſize of the bottle, and the nature of the water made uſe of is greater; for when I uſe the ſmall bottle which holds three meaſures, and fill it with diſtilled water, the uſual diminution in trying common air is 1.08; whereas, if I fill the bottle with water from my tub, the diminution is uſually about .05 leſs. If I uſe the bottle which holds twelve meaſures, filled with diſtilled water, the diminution is about 1.15; and if I uſe the ſame bottle, filled with water from my tub, about 1.08.

The reaſon of this difference is, that water has a power of abſorbing a ſmall quantity of nitrous air; and the more dephlogiſticated the water is, the more of this air it can abſorb. If the water is of ſuch a nature alſo as to froth or form bubbles on letting in the common air, the diminution is remarkably leſs than in other water.

The following table contains the diminution produced in trying common air in the bottle containing three meaſures, with ſeveral different kinds of water, and alſo the diminution which the ſame quantity of nitrous air ſuffered by being only ſhook in the ſame bottle, without the addition of any common air, tried by ſtopping the mouth of the bottle with my finger, and ſhaking it briſkly for one minute, and afterwards for one minute more.

Diminution in trying common air.	Diminution on ſhaking nitrous air for		
	one minute	two minutes.	
1.099	.118	.122	Diſtilled water.
1.049	.083	.088	Water from tub.
1.036	.090	.098	Pump water.
1.062	.090	.099	Diſtilled water, in which a few drops of liver of ſulphur were kept for a few days.
1.045	.052	.056	Diſtilled water impregnated with nitrous air, by keeping it with about $\frac{1}{4}$ of its bulk of nitrous air for two days, and frequently ſhaking it.
.897	.082	.085	Water fouled by oak ſhavings. N. B. it frothed very much.

In general, the diminution was nearly as great with rain water as diſtilled water; but ſometimes I have found rain water froth a good deal, and then the diminution was not much greater than by the water fouled with oak ſhavings.

This difference in the diminution, according to the nature of the water, is a very great inconvenience, and ſeems to be the chief cauſe of uncertainty in trying the purity of air; but it is by no means peculiar to this method, as I have found as great a difference in FONTANA's method, according as I have filled the tube with different waters*. But it ſhews plainly, how little all the experiments which have hitherto been made for determining the variations in the purity of the atmoſphere can be relied on, as I do not know that any one before has been attentive to the nature of the water he has uſed, and the difference proceeding from the difference of waters is much greater than any I have yet found in the purity of air.

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* I do not find that it makes much difference in FONTANA's method whether the water is diſpoſed to froth or not; but the advantage which it has in that reſpect over this method is not of much conſequence, as it is eaſy finding water which will not froth.

The beſt way I know of obviating this inconvenience is to be careful always to uſe the ſame kind of water: that which I always uſe is diſtilled, as being moſt certain to be always alike. I ſhould have uſed rain water, as being eaſier procured, if it had not been that this water is ſometimes apt to froth, which I have never known diſtilled water do.

As I found that the power with which the diſtilled water I uſed abſorbed nitrous air was greater at ſome times than others, which muſt neceſſarily make an error in the obſervation, I was in hopes that, by obſerving the quantity of nitrous air which the water abſorbed in the ſame manner as in the preceding experiment, together with the heat of the water, as that alſo ſeems to affect the experiment, one might be able to correct the obſerved teſt, and thereby obviate the error which would otherwiſe ariſe from any little difference in the nature of the water employed. With this view I made the following experiment.

I purged ſome diſtilled water of its air by boiling, and kept one part of it for a week in a bottle along with ſome dephlogiſticated air, and ſhook it frequently; the other part was treated in the ſame manner with phlogiſticated air. At the end of this time I found, by a mean of three different trials, that the teſt of common air tried with the firſt of theſe waters was 1.139, the diminution which nitrous air ſuffered by being ſhook 2′ in it in the uſual manner was .285. The teſt of the ſame air tried with the laſt of theſe waters was only 1.054, and the diminution of nitrous air only .090, the heat of the water in the tub and of the diſtilled waters being 45°. I then raiſed both the water of the tub and the diſtilled waters to the heat of 67°, and found that the teſt of the ſame air, tried by the firſt water, was then 1.100, and by the latter 1.044; and that the diminution of nitrous air was .235 by the firſt water, and .089 by the latter.

It ſhould ſeem from hence, as if the obſerved teſt ought to be corrected by ſubtracting $\frac{4}{10}$ths of the diminution which nitrous air ſuffers by being ſhaken in the water, and adding .002 for every 3° of heat above 0, as the foregoing trials will agree very well together, if they are corrected by this rule, and better than if corrected by any different rule, as will appear by the following table.

	Heat.	Diminution of nitrous air.	Obſerved teſt.	Correction for		Corrected teſt.
				Diminution.	Heat.	
Former water	45	.285	1.139	.114	.030	1.055
	67	.235	1.100	.094	.045	1.051
Latter water	45	.090	1.054	.036	.030	1.048
	67	.089	1.044	.036	.045	1.053

Though in all probability this correction will diminiſh the error proceeding from a difference in the nature of the diſtilled water employed, yet I have reaſon to think, that it will by no means entirely take it away; for which reaſon I do not in general make uſe of it. In almoſt all the trials, indeed, in which I have applied the correction, it has come out very nearly the ſame; which ſeems to ſhew, that there was no other difference in the abſorbing power of the diſtilled water I employed, than what proceeded from its difference of heat. The above experiment, however, ſhews plainly, that diſtilled water is capable of a very great difference in this reſpect independent of its heat.

In the ſecond method of proceeding, or that in which the nitrous air is added to the reſpirable, I found nearly the ſame difference in the diminution, according as the bottle was ſhaken briſkly or gently, as in the former method: I found alſo nearly the ſame difference, or perhaps rather leſs, according to the nature of the water employed, only it ſeemed to be of not much conſequence whether the water frothed or not; but there ſeemed to be much leſs difference in the diminution, according to the time which the air took up in paſſing into the bottle. The uſual diminution on trying common air with different quantities of nitrous air, when diſtilled water was employed, was as follows:

Common air.	Nitrous air.	Diminution.
1.	.6	.74
	.8	.88
	1.	.89
	1.5	.90

It appears, therefore, that $\frac{8}{10}$ths of a meaſure of nitrous is ſufficient to produce very nearly the full diminution. I chuſe, however, always to uſe one meaſure. It appears alſo, that the diminution is always much leſs in this method than when the common air is added to the nitrous; as in that method it was before ſaid, that the uſual diminution was 1.08. The reaſon of this is, that when nitrous and common air are mixed together, the nitrous air is robbed of part of its phlogiſton, and is thereby turned into phlogiſticated nitrous acid, and is abſorbed by the water in that ſtate, and beſides that, the common air is phlogiſticated, and thereby diminiſhed: ſo that the whole diminution on mixing is equal to the bulk of nitrous air, which is turned into acid, added to the diminution which the common air ſuffers by being phlogiſticated. Now it appears, that when a ſmall quantity of nitrous air comes in contact with a large quantity of common air, it is more completely deprived of its phlogiſton, and is abſorbed by the water in a more dephlogiſticated ſtate than when a ſmall quantity of common air comes in contact with a large quantity of nitrous; conſequently, in the ſecond method, where ſmall portions of nitrous air come in contact with a large quantity of common air, the nitrous air is more deprived of its phlogiſton, and therefore a leſs quantity of it is required to phlogiſticate the common air than in the firſt method, where ſmall portions of common air come in contact with a large quantity of nitrous air; ſo that a leſs quantity of the nitrous air is abſorbed in the ſecond method than in the firſt. As to the common air, as it is completely phlogiſticated in both methods, it moſt likely ſuffers an equal diminution in both.

A clear proof that a leſs quantity of nitrous is required to phlogiſticate a given quantity of common air in the ſecond method than in the firſt, is, that if common air is mixed with a quantity of nitrous air not ſufficient to completely phlogiſticate it, the mixture will be more phlogiſticated if the nitrous air is added ſlowly to the common, as in the ſecond method, than if the common air is added to the nitrous; and if the nitrous air is added ſlowly to the common, without being in contact with water, the mixture will be found to be ſtill more phlogiſticated than in the ſecond method, where the two airs are in contact with water at the time of mixing.

The following table contains the reſult of the experiments I have made on this ſubject.

Firſt method.			Second method.			Nitrous air added ſlowly to common without being in contact with water.		
Nitrous air.	Bulk of mixture	Teſt.	Nitrous air.	Bulk of mixture	Teſt.	Nitrous air.	Bulk of mixture	Teſt.
.716	.856	.244	.635	.849	.137	.294	.836	.337
.474	915	.513	.430	.867	.352			
			.280	.930	.599			

The two-firſt ſets of experiments were not tried with the apparatus above deſcribed, as that held too ſmall a quantity, but with another upon the ſame principle. The laſt ſet was tried by the apparatus repreſented in fig. 4. where A is a bottle containing nitrous air, inverted into the tub of water DE; B is a bottle with a bent glaſs tube C fitted to its mouth. This bottle is filled with common air, without any water, and is firſt ſlightly warmed by the hand; the end of the glaſs tube is then put into the bottle of nitrous air, as in the figure; conſequently, as the bottle B cools, a little nitrous air runs into it, which, by the common air in it, is deprived of its elaſticity, ſo that more nitrous air runs in to ſupply its place. By this means the nitrous air is added ſlowly to the common without coming in contact with water, till the whole of the nitrous air has run out of the bottle A into B; then, indeed, the water runs through the glaſs tube into B, to ſupply the vacancy formed by the diminution of the common air.

It appears from the foregoing table, that a quantity of nitrous air, uſed in the firſt method, does not phlogiſticate common air more than three-fourths of that quantity uſed in the ſecond way does, and not ſo much as half that quantity uſed in the third way: ſo that we may ſafely conclude, that it is this circumſtance of the nitrous air going further in phlogiſticating common air in ſome circumſtances than others, which is the cauſe that the diminution in trying the purity of air by the nitrous teſt is ſo much greater in ſome methods of mixing them than in others.

From what was ſaid in p. 119. it ſhould ſeem as if the ſecond method was more exact than the firſt, as the error proceeding from the air employing more or leſs time in paſſing into the bottle was found to be leſs, and that proceeding from a difference in the water, and from the bottle being ſhaken more or leſs ſtrongly was not greater. I, however, have found, that the trials of the ſame air on the ſame day have commonly differed more when made in this manner than in the firſt; for which reaſon, and becauſe in trying common air the firſt method takes up the leaſt time, I have commonly uſed that.

It ſhould be obſerved, that in trying dephlogiſticated air by the firſt method it is convenient to uſe different bottles, according to the different purity of the air; and the ſame air will appear purer, if tried by a larger bottle than by a ſmaller. For example, if its teſt, tried by the large bottle, comes out 2.54, it will appear not more than 2.44, if tried by the middle bottle; and, in like manner, if its teſt by the middle bottle comes out 1.11, it will appear to be about 1.08, if tried by the leaſt bottle; for this reaſon it is right always to ſet down which bottle it is tried by.

I think I may confidently aſſert, that either of the above methods are conſiderably more accurate than FONTANA's, ſuppoſing the experiment to be made exactly in his manner, that is, determining the quantities by meaſure. But, in order to judge which method of mixing the airs is moſt exact, it was neceſſary to determine the quantities in his method alſo by weight, as otherwiſe it would be uncertain whether my method of mixing the airs is really better than his, or whether the apparent greater exactneſs proceeds only from the ſuperiority of weighing above meaſuring: for this reaſon I made ſome experiments in which common and nitrous air were mixed in his manner, except that I uſed only one meaſure of each, as Dr. INGEN-HOUSZ did, and that the nitrous air was put up firſt, the true diminution being determined by weight, by firſt weighing the tube under water with the nitrous air in it, and then adding the common air, and weighing the tube again under water. It was unneceeſſary, for the reaſons given in p. 110. and 112. to determine the quantity of either the nitrous or common air by weight. My reaſon for this variation was, that it afforded a much eaſier method of determining the quantities by weight, was leſs trouble, and, I believe, muſt be at leaſt as exact: for I have always found, that the experiments made with the Abbé FONTANA's apparatus, in which I uſed only one meaſure of each air, agreed better together than thoſe in which I uſed two of common, and added the nitrous air by one at a time; and I imagine it can be of no ſignification whether the nitrous or common air is put in firſt, as I cannot perceive the diminution to be ſenſibly greater in one of thoſe ways than the other*.

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* It is not extraordinary, that in this method the diminution is juſt the ſame whether the common or nitrous air is put up firſt, notwithſtanding that in mine it is very different; ſince in this method the two airs mix in the ſame manner whichever is put up firſt: whereas in mine, the manner in which they mix is very different in thoſe two caſes; as in one, ſmall portions of common air come in contact with large portions of the nitrous; and in the other, ſmall portions of nitrous air come in contact with large portions of common air.

From the reſult of theſe experiments I am perſuaded, that my method of mixing the airs is really rather more accurate than FONTANA's, as in trying the ſame bottle of air ſix or ſeven times in my method the different trials would not often differ more than $\frac{1}{200}$dth part, and very ſeldom more than $\frac{1}{100}$dth; whereas in his there would commonly be a difference of $\frac{1}{100}$dth, and frequently near twice that quantity, though I endeavoured to be as regular as I could in my manner of trying the experiment. My method alſo certainly requires leſs dexterity in the operator than his.

It is of much importance towards forming a right judgement of the degree of accuracy to be expected in the nitrous teſt, to know how much it is affected by a difference in the nitrous air employed. Now it muſt be obſerved, that nitrous air may differ in two reſpects; firſt, it may vary in purity, that is, in being more or leſs mixed with phlogiſticated or other air; and, ſecondly, it is poſſible, that out of two parcels equally pure one may contain more phlogiſton than the other. If it differs in the ſecond reſpect, it will evidently cauſe an error in the teſt, in whatever proportion it is mixed with the reſpirable air; but if it differs only in the firſt reſpect, it will hardly cauſe any ſenſible error, unleſs it is more than uſually impure, provided care is taken to uſe ſuch a quantity as is ſufficient to produce the full diminution. This has been obſerved by the Abbé FONTANA, and agrees with my own experiments; for the teſt of common air tried in my uſual method, with ſome nitrous air which had been debaſed by the mixture of common air, came out only 18 thouſandths leſs than when tried with air of the beſt quality, though this air was ſo much debaſed that the diminution, on mixing two parts of this with five of common, was one-ſixth part leſs than when good nitrous air was employed; which ſhews, that the error proceeding from the difference of purity of the nitrous air is much leſs when it is uſed in the full quantity than in a ſmaller proportion; and alſo ſhews, that if it is uſed in the full quantity it can hardly cauſe any ſenſible error, unleſs it is more impure than uſual. One does not eaſily ſee, indeed, why it ſhould cauſe any error; for no reaſon appears why the mixture of phlogiſticated or other air, not abſorbable by water, and not affected by reſpirable air, ſhould prevent the nitrous air from diminiſhing and being diminiſhed by the reſpirable air in juſt the ſame manner that it would otherwiſe be. It muſt be obſerved, however, that if the nitrous air is mixed with fixed air, it will cauſe an error, as part of the fixed air will be abſorbed by the water while the teſt is trying; for which reaſon care ſhould be taken that the nitrous air ſhould not be much mixed with this ſubſtance, which it will hardly be, unleſs either the metal it is procured from is covered with ruſt; or unleſs the water in which it is received contains much calcareous earth ſuſpended by fixed air, as in that caſe, if any of the nitrous acid comes over with the air, it will diſſolve the calcareous earth, and ſeparate ſome fixed air.

In order to ſee whether it is poſſible for nitrous air to differ in the ſecond reſpect, I procured ſome from quickſilver, copper, braſs, and iron, and obſerved the teſt of the ſame parcel of common air with them, on the ſame day, making four trials with each, when the difference between the teſts tried with the three firſt kinds of air was not greater than might proceed from the error of the experiment; but thoſe tried with the air from iron were $\frac{15}{1000}$ths greater than the reſt. I then took the teſt of ſome more common air with them in the ſame manner, only uſing four parts of common to one of nitrous air, when the teſts tried with the air from iron came out ſmaller than the reſt by not leſs than $\frac{130}{1000}$ths. It ſhould ſeem, therefore, from theſe experiments, that the nitrous air procured from iron, beſides being much more impure than the others, differs from them alſo in the ſecond reſpect; that is, that the pure nitrous air in it contains rather leſs phlogiſton than that in the others: whence it happens, that a greater quantity is neceſſary to phlogiſticate a given portion of common air, and conſequently that the diminution is greater when a ſufficient quantity of it is uſed, though with a leſs proportion the diminution is much leſs than with other nitrous air, on account of its greater impurity. As for the air procured from the three other ſubſtances, I cannot be ſure that there is any difference between them. The nitrous air I always uſe is made from copper, as it is procured with leſs trouble than from quickſilver, and I have no reaſon to think it more likely to vary in its quality.

During the laſt half of the year 1781, I tried the air of near 60 different days, in order to find whether it was ſenſibly more phlogiſticated at one time than another; but found no difference that I could be ſure of, though the wind and weather on thoſe days were very various; ſome of them being very fair and clear, others very wet, and others very foggy.

My way was to fill bottles with glaſs ſtoppers every now and then with air from without doors, and preſerve them ſtopped and inverted into water, till I had got ſeven or eight, and then take their teſt; and whenever I obſerved their teſt, I filled two bottles, one of which was tried that day, and the other was kept till the next time of trying, in order to ſee how nearly the teſt of the ſame air, tried on different days, would agree. The experiment was always made with diſtilled water, and care was always taken to obſerve the diminution which nitrous air ſuffered by being ſhaken in the water, as mentioned in p. 115. The heat of the water in the tub alſo was commonly ſet down. Moſt of the bottles were tried only in the firſt method; but ſome of them were alſo tried by the ſecond, and by the method juſt deſcribed in the manner of FONTANA.

The reſult was, that the teſt of the different bottles tried on the ſame day never differed more than .013, and in general not more than half that quantity. The teſt, indeed, of thoſe tried on different days differed rather more; for taking a mean between the teſts of the bottles tried on the ſame day, there were two of thoſe means which differed .025 from each other; but, except thoſe two, there were none which differed more than .013. Though this difference is but ſmall, yet as each of theſe means is the mean of ſeven or eight trials, it is greater than can be expected to proceed from the uſual errors of the experiment. This difference alſo is not much diminiſhed by correcting the obſervations on account of the heat and abſorbing power of the water, according to the rule in p. 118. This might incline one to think, that the parcels of air examined on ſome of thoſe days of trial were really more dephlogiſticated than the reſt; but yet, I believe, that they were not: for whenever there was any conſiderable difference between the means of two ſucceſſive days of trial, there was nearly the ſame difference between the teſts of the two bottles of the very ſame air tried on thoſe two days. For example, the mean of the trials on July 7. was .016 leſs than that of thoſe on the 15th of the ſame month; but then the teſt of the air caught and tried on the 7th was equally leſs than that of the air of the ſame day tried on the 15th; which ſhews, that this difference between the means of thoſe two days was not owing to the parcels of air tried on the former day being really more dephlogiſticated than thoſe tried on the latter, but only to ſome unperceived difference in the manner of trying the experiment; or elſe to ſome unknown difference in the nature of the water or nitrous air employed. A circumſtance which ſeems to ſhew that it was owing to the firſt of theſe two cauſes is, that it frequently happened, that on thoſe days in which the teſts taken in the firſt method came out greater than uſual, thoſe taken in FONTANA's manner, or in the ſecond method, did not do ſo; the trials, however, made in theſe two methods were too few to determine any thing with certainty. On the whole there is great reaſon to think, that the air was in reality not ſenſibly more dephlogiſticated on any one of the ſixty days on which I tried it than the reſt.

The higheſt teſt I ever obſerved was 1.100, the loweſt 1.068, the mean 1.082.

I would by all means recommend it to thoſe who deſire to compare the air of different places and ſeaſons, to fill bottles with the air of thoſe places, and to try them at the ſame time and place, rather than to try them at the time they were filled, as all the errors to which this experiment is liable, as well thoſe which proceed from ſmall differences in the manner of trying the experiment, as thoſe which proceed from a difference in the nature of the water and nitrous air, will commonly be much leſs when the different parcels of air are tried at the ſame time and place than at different ones; provided only, that air can be kept in this manner a ſufficient time without being injured, which I believe it may, if the bottles are pretty large, and care is taken that they, as well as the water uſed in filling them with air, are perfectly clean. I have tried air kept in the abovementioned manner for upwards of three-quarters of a year in bottles holding about a pint, which I have no reaſon to think was at all injured; but then I have tried ſome kept not more than one-third part of that time which ſeemed to have been a little impaired, though I do not know what it could be owing to, unleſs it was that the bottles were ſmaller, namely, holding leſs than one-fourth of a pint, and that in all of them, except two, which were ſmaller than the reſt, the ſtopper which, however, fitted in very tight, was tied down by a piece of bladder.

I made ſome experiments alſo to try whether the air was ſenſibly more dephlogiſticated at one time of the day than another, but could not find any difference. I alſo made ſeveral trials with a view to examine whether there was any difference between the air of London and the country, by filling bottles with air on the ſame day, and nearly at the ſame hour, at Marlborough-ſtreet and at Kenſington. The reſult was, that ſometimes the air of London appeared rather the pureſt, and ſometimes that of Kenſington; but the difference was never more than might proceed from the error of the experiment; and by taking a mean of all, there did not appear to be any difference between them. The number of days compared was 20, and a great part of them taken in winter, when there are a greater number of fires, and on days when there was very little wind to blow away the ſmoke.

It is very much to be wiſhed, that thoſe gentlemen who make experiments on factitious airs, and have occaſion to aſcertain their purity by the nitrous teſt, would reduce their obſervations to one common ſcale, as the different inſtruments employed for that purpoſe differ ſo much, that at preſent it is almoſt impoſſible to compare the obſervations of one perſon with thoſe of another. This may be done, as there ſeems to be ſo very little difference in the purity of common air at different times and places, by aſſuming common air and perfectly phlogiſticated air as fixed points. Thus, if the teſt of any air is found to be the ſame as that of a mixture of equal parts of common and phlogiſticated air, I would ſay, that it was half as good as common air; or, for ſhortneſs, I would ſay, that its ſtandard was $\frac{1}{2}$: and, in general, if its teſt was the ſame as that of a mixture of one part of common air and x of phlogiſticated air, I would ſay, that its ſtandard was $\frac{1}{1+x}$. In like manner, if one part of this air would bear being mixed with x of phlogiſticated air, in order to make its teſt the ſame as that of common air, I would ſay, that it was $1+x$ times as good as common air, or that its ſtandard was $1+x$; conſequently, if common air, as Mr. SCHEELE and LA VOISIER ſuppoſe, conſiſts of a mixture of dephlogiſticated and phlogiſticated air, the ſtandard of any air is in proportion to the quantity of pure dephlogiſticated air in it. In order to find what teſt on the Eudiometer anſwers to different ſtandards below that of common air, all which is wanted is to mix common and perfectly phlogiſticated air in different proportions, and to take the teſt of thoſe mixtures; but in ſtandards above that of common air, it is neceſſary to procure ſome good dephlogiſticated air, and to find its ſtandard by trying what proportion of phlogiſticated air it muſt be mixed with, in order to have the ſame teſt as common air, and then to mix this dephlogiſticated air with different proportions of phlogiſticated air, and find the teſt of thoſe mixtures*.

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* The rule for computing the ſtandard of any mixture of dephlogiſticated and phlogiſticated air is as follows. Suppoſe that the teſt of a mixture of D parts of dephlogiſticated air with P of phlogiſticated air is the ſame as that of common air,

On this principle I found the ſtandard anſwering to different teſts on both my Eudiometers, and alſo on FONTANA's, to be as follows:

Standard.	Teſt by firſt method.	Teſt by ſecond method	Teſt by FONTANA abridged. 1	2	3	4	Total diminu.
4.8	5.02 – –	3.62	.73	.44	.13	1.02	3.98
3.61	3.72 – –	2.70	.75	.49	1.00	– –	3.
2.39	2.55 by large bottle 2.45 by middle bottle	1.87	.76	.96	1.92	– –	2.08
1.00	1.11 by middle bottle 1.08 by leaſt bottle	.89	1.00	– –	– –	– –	1.00
.75	.81 – –	.69	1.23	– –	– –	– –	.77
.5	.57 – –	.51	1.45	– –	– –	– –	.55
.25	.32 – –	.31	1.66	– –	– –	– –	.34
.0	.07 – –	.08	1.94	– –	– –	– –	.06

Standard.	Teſt by FONTANA's method. 1	2	3	4	5	6	7	Total diminu.
4.8	1.75	1.43	1.11	.78	.46	.21	1.18	7.82
3.61	1.75	1.46	1.17	.89	1.16	2.13	– –	5.87
2.39	1.76	1.50	1.25	2.06	– –	– –	– –	3.94
1.	1.81	2.12	3.12	– –	– –	– –	– –	1.88
.75	1.82	2.54	– –	– –	– –	– –	– –	1.46
.5	1.98	2.94	– –	– –	– –	– –	– –	1.06
.25	2.42	3.39	– –	– –	– –	– –	– –	.61
.0	2.91	– –	– –	– –	– –	– –	– –	.09

The phlogiſticated air uſed in theſe experiments was procured by means of liver of ſulphur.

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air, then is the ſtandard of the dephlogiſticated air $\frac{D+P}{D}$. Let now δ parts of this dephlogiſticated air be mixed with φ parts of phlogiſticated air, the ſtandard of the mixture will be $\frac{D+P}{D} \times \frac{\delta}{\delta+\varphi}$.

The trials, called FONTANA abridged, were made in the Abbé FONTANA's manner, except that only one meaſure of reſpirable air was uſed, the nitrous air being added by one meaſure at a time as uſual. The column marked 1 at top is the bulk of the mixture after one meaſure of nitrous air was added; that marked 2, its bulk after two meaſures were added, and ſo on.

It muſt be obſerved, that in theſe experiments a conſiderable diminution took place in taking the teſt of the unmixed phlogiſticated air, or that whoſe ſtandard is marked 0 in the table; but, notwithſtanding this, the air, as far as I could perceive, was perfectly phlogiſticated, the diminution being cauſed merely by the abſorbtion of the nitrous air by the water. What ſhews this to be the caſe is, that if common and nitrous air are mixed in ſuch proportions as that the nitrous ſhould be predominant, ſo as to be conſiderably diminiſhed by the mixture of common air, this mixture will produce as great a diminution with nitrous air as the phlogiſticated air uſed in theſe experiments; and if plain nitrous air is added to nitrous air, the diminution is ſtill greater. This ſhews, that a conſiderable diminution is produced by mixing perfectly phlogiſticated air with nitrous air, and alſo that air may be perfectly phlogiſticated by liver of ſulphur.

Theſe experiments alſo ſhew the neceſſity of uſing ſuch a quantity of nitrous air as is ſufficient to produce the full diminution, in order to form a proper eſtimate of the goodneſs of air; for if the quantity of nitrous air is much leſs than that, the air you try will appear very little better than air of a much inferior quality. For example, if in taking the teſt of very good dephlogiſticated air, only an equal bulk of nitrous air is uſed, it will appear very little better than a mixture of equal parts of this and phlogiſticated air; and if twice that quantity of nitrous air is uſed, it will appear very little better than a mixture of three parts of this air with one of phlogiſticated. Another great advantage of uſing the full quantity of nitrous air is, that thereby the error ariſing from any difference in its purity is very much diminiſhed.

Perfectly phlogiſticated air may be conveniently procured by putting ſome ſolution of liver of ſulphur into a bottle of air well ſtopped, and ſhaking it frequently till the air is no longer diminiſhed, which, unleſs it is ſhaken very frequently, will take up ſome days. Care muſt be taken, however, to looſen the ſtopper now and then, ſo as to let in air to ſupply the place of the diminiſhed air. In order to know when the air is as much diminiſhed as it can be, the beſt way is, when the air is ſuppoſed to be nearly phlogiſticated, to place the bottle with its mouth under water, ſtill keeping it ſtopped, and to looſen the ſtopper now and then, while under water, ſo as to let in water to ſupply the place of the diminiſhed air, by which means the alteration of weight of the bottle ſhews whether the air is diminiſhed or not. If the ſolution of liver of ſulphur is made by boiling together fixed alkali, lime, and flowers of ſulphur, which is the moſt convenient way of procuring it, the air phlogiſticated by it will be perfectly free from fixed air: whether it will be ſo if the liver of ſulphur is made without lime, I am not ſure.

A ſtill more convenient way, however, of procuring phlogiſticated air is by a mixture of iron filings and ſulphur; and, as far as I can perceive, the air procured this way is as completely phlogiſticated as that prepared by liver of ſulphur.

Where the impurities mixed with the air have any conſiderable ſmell, our ſenſe of ſmelling may be able to diſcover them, though the quantity is vaſtly too ſmall to phlogiſticate the air in ſuch a degree as to be perceived by the nitrous teſt, even though thoſe impurities impart their phlogiſton to the air very freely. For inſtance, the great and inſtantaneous power of nitrous air in phlogiſticating common air is well known; and yet ten ounce meaſures of nitrous air, mixed with the air of a room upwards of twelve feet each way, is ſufficient to communicate a ſtrong ſmell to it, though its effect in phlogiſticating the air muſt be utterly inſenſible to the niceſt Eudiometer; for that quantity of nitrous air is not more than the 140000th part of the air of the room, and therefore can hardly alter its teſt by more than $\frac{3}{140000}$ or $\frac{1}{47000}$th part. Liver of ſulphur alſo phlogiſticates the air very freely, and yet the air of a room will acquire a very ſtrong ſmell from a quantity of it vaſtly too ſmall to phlogiſticate it in any ſenſible degree. In like manner it is certain, that putrifying animal and vegetable ſubſtances, paint mixed with oil, and flowers, have a great tendency to phlogiſticate the air; and yet it has been found, that the air of an houſe of office, of a freſh painted room, and of a room in which ſuch a number of flowers were kept as to be very diſagreeable to many perſons, was not ſenſibly more phlogiſticated than common air. There is no reaſon to ſuppoſe from theſe inſtances, either that theſe ſubſtances have not much tendency to phlogiſticate the air, or that nitrous air is not a true teſt of its phlogiſtication, as both theſe points have been ſufficiently proved by experiment; it only ſhews, that our ſenſe of ſmelling can, in many caſes, perceive infinitely ſmaller alterations in the purity of the air than can be per-

Fig. 4.

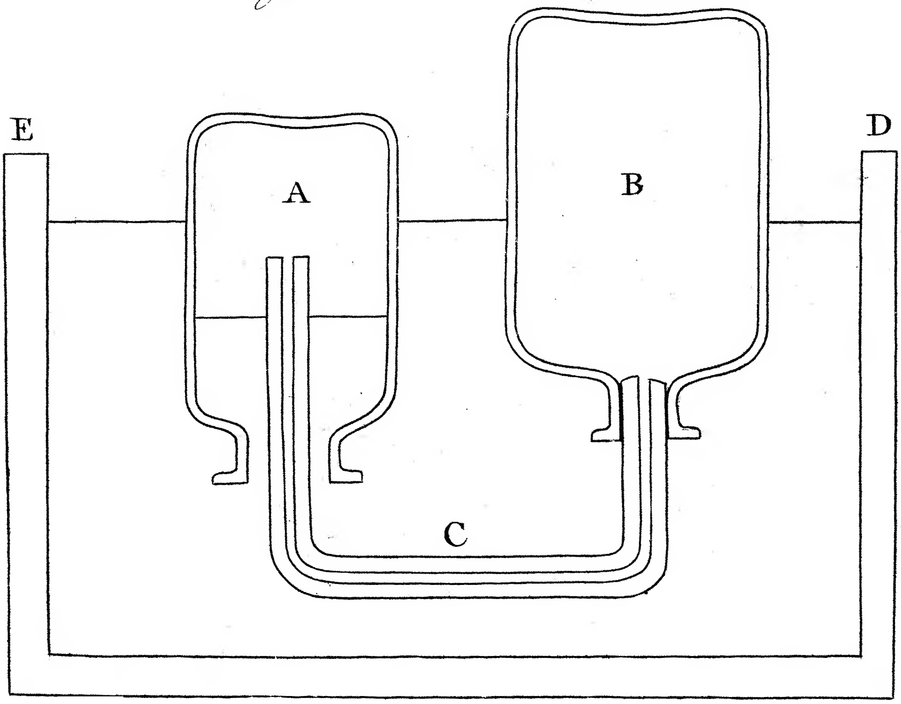


Fig.1.

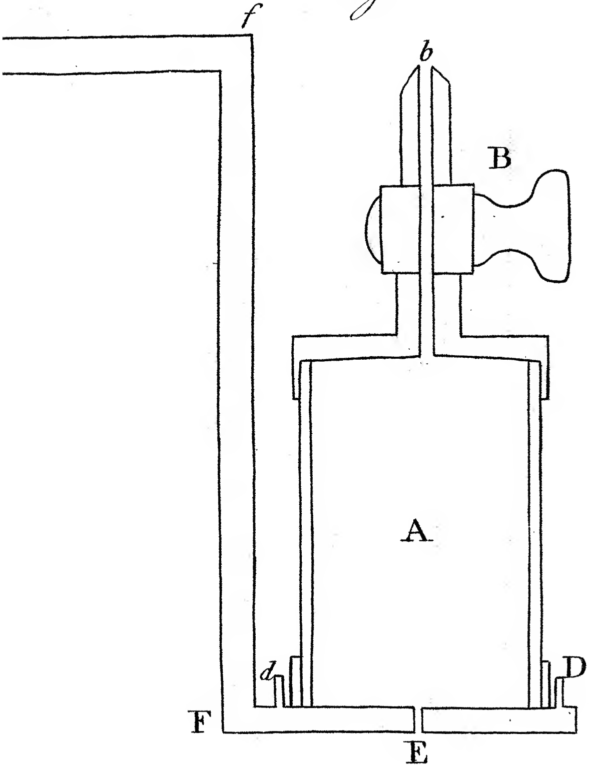


Fig.2.

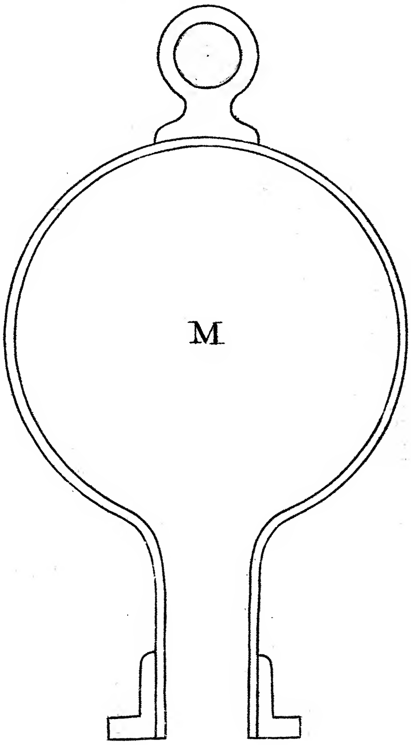
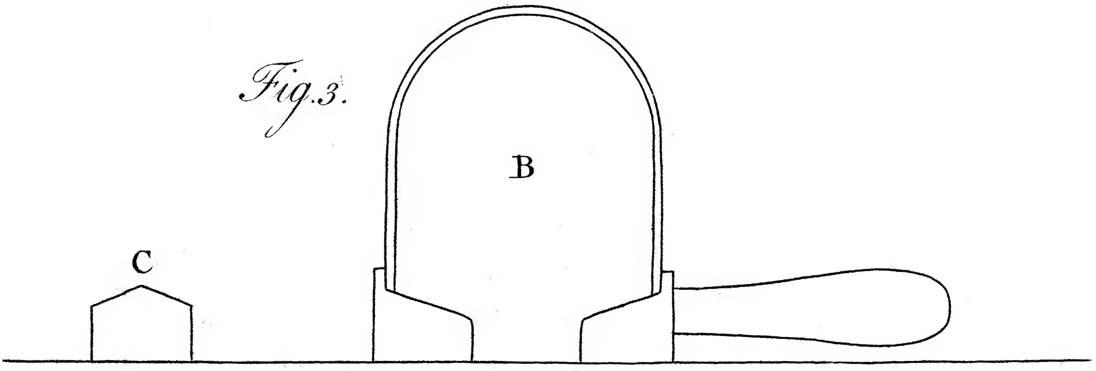


Fig.3.



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ceived by the nitrous teſt, and that in moſt rooms the air is ſo frequently changed, that a conſiderable quantity of phlogiſticating materials may be kept in them without ſenſibly impairing the air. But it muſt be obſerved, that the nitrous teſt ſhews the degree of phlogiſtication of air, and that only; whereas our ſenſe of ſmelling cannot be conſidered as any teſt of its phlogiſtication, as there are many ways of phlogiſticating air without imparting much ſmell to it; and, I believe, there are many ſtrong ſmelling ſubſtances which do not ſenſibly phlogiſticate it.

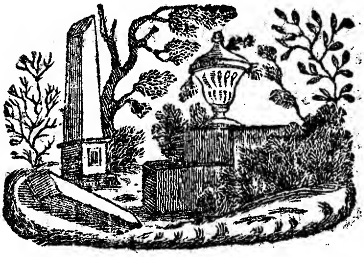

Fig 4.

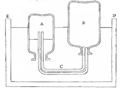

Fig 1.

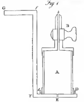

Fig 2.

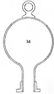

Fig 3.